THE PLAINS ANTHROPOLOGIST

FRANKLIN FENENGA, EDITOR NUMBER 6, APRIL, 1956

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A MEDIUM FOR THE PUBLICATION OF THE RESULTS OF ANTHROPOLOGICAL AND RELATED RESEARCHES IN THE PLAINS

A FOLSOM SITE IN IDAHO

John Martin Campbell

During the summer of 1955 the writer learned that a Folsom point had been discovered near the Snake River in southern Idaho. The collector, Mr. James H. Beck of Pocatello, Idaho was contacted, and through his courtesy, arrangements were made to examine the point and the site of discovery.

The point appears to be representative of the Folsom Fluted type in size and method of construction. It measures 39 mm. from the center of the base to the tip, and its greatest width is 24 mm. The fluting on either side is distinct, the length of the longest flute being 30 mm. The marginal retouching is well executed, and the basal projections are clearly defined. The stone is apparently quartzite, nearly opaque, and of a light gray color. (Fig. 1).

This Folsom point was discovered by Mr. Beck in 1950, on the surface, two miles north of the Snake River, and approximately fifteen miles southwest of the town of American Falls, Idaho. The site is situated in a basaltic coulee known as "Lake Channel," one of several similar coulees in the area which trend southward toward the Snake. High sand dunes occupy a portion of the upper western side of Lake Channel. The point was discovered lying on a hard clay layer which had been recently exposed between two of the dunes. No other artifacts were found in association.

An effort was made by Mr. Beck and the writer to trace the extent of the clay layer upon which the Folsom point was discovered, and which is of varying thickness. This was difficult because of the intervening dunes, but the layer appears to follow the contour of the upper western side of the coulee, to finally disappear under a cap of basalt approximately 15 feet thick. Here three bones were discovered protruding from the clay layer, which at this point lay well under a basalt boulder 12 feet in height (Fig. 2). Two of the bones are too fragmentary for identification. The third is a complete metacarpal, undoubtedly bison, although not specifically identifiable. (This bone was examined and identified as bison by Dr. J. T. Gregory, Department of Geology, Yale University). The metacarpal was heavily mineralized, as were the two smaller fragments. *In situ* with the bones, and also well imbedded in the clay layer, was a single spall of brown quartzite, sharply fractured on two faces, but showing no other evidence of working. These bones and the spall were discovered 75 yards west of the location of the Folsom point.

Mr. Beck has previously collected several small tanged or notched points in the general area. These are mostly of obsidian, although metal points have also been found in Lake Channel. However, the points of metal and obsidian have, in almost all instances, been recovered from the dunes, or from the sandy soil surrounding the dune area, rather than from the lower clay layer where the Folsom point was discovered.

The site area evidently contains at least two cultural levels. The upper consists of sandy soil or dune sand containing both historic, and probably late prehistoric artifacts represented by the small notched or tanged obsidian points and metal points. Below this level is a clay layer ranging in thickness from several inches to an unknown depth. It lies at least partially under a basalt cap. Mineralized bison bone, and what appears to be a deliberately broken spall

of quartzite, were found in this clay layer. The Folsom point was discovered lying directly on the surface of the clay, and in view of the discovery of the bones and spall, I think it reasonable to assume that this point belonged with the clay layer rather than that it had reached this position from some higher level in the sandy soil above.

Lake Channel Site probably represents an extension of the Folsom complex found to the south and east.

Yale University

Figure 1.

Figure 2.

OPPORTUNITIES AND PITFALLS FOR THE LINGUISTIC FIELDWORKER

Paul Schach

The purpose of this paper is twofold: I wish first of all to point out the opportunities for linguistic investigation in this country and to emphasize the necessity of greatly increasing the research now being done in this field; and I want secondly to discuss a few characteristic traps which are ever ready to ensnare the unwary researcher. Although most of my comments are intended for ambitious neophytes, some of the examples used to illustrate various points may be of interest also to the expert. These examples have been drawn partly from my own field records and partly from recent publications by specialists. Actually I could very easily have limited my illustrative material to "bloopers" I have made. My only reason for honoring some of my esteemed colleagues (nearly all of whom are also good friends of mine) in this odd manner is to impress upon the beginner in this field the importance of checking and rechecking his findings. At the same time it may be consoling in a negative sort of way to know that even experienced linguists sometimes tumble into the pit.

Opportunities for linguistic fieldwork within the borders of the United States and Canada are manifold. The many native languages, regional variations in English, old-established non-English dialects (such as Canadian and Louisiana French, the Spanish of the Southwest, and Pennsylvania German), and more recent American immigrant speech all present enticing and challenging problems for the linguist. In recent years the significant and complex subject of multilingualism, so long neglected, has begun to receive the attention it deserves from competent students of language. The process of linguistic acculturation, of the transition of individuals, families, and entire speech-communities from a non-English tongue to English, can still be observed almost everywhere. Unfortunately this language shift has been so greatly accelerated by social pressures of various kinds, by the public schools, and by the overwhelming leveling effect of radio, the talking pictures, and television that the possibilities of observing the actual clash and conflict of languages within a family or larger social groups will be considerably diminished in the near future. The opportunities for linguistic research which are so numerous and varied today are disappearing swiftly and irretrievably through the rapid obsolescence of both indigenous and imported non-English tongues. The significance of the investigation of these vanishing languages has been stated clearly in Bulletin No. 34 of the American Council of Learned Societies: "Conference on Non-English Speech in the United States" (Washington, 1942). Obviously the urgency of recording immediately those languages which have not yet been exhaustively studied can scarcely be exaggerated. While this is primarily a task for the linguist, there are many phases of the problem which are of interest also to the anthropologist, the psychologist, and the sociologist. At the present time in Nebraska, studies are being made of the Omaha and Ponca languages and of several German dialects. It is hoped that the necessary time and money can be found to intensify these investigations and to extend them to include all the native and immigrant languages represented in and around

this state.

Commensurate with the manifold opportunities for linguistic research are the multifarious pitfalls into which, in unguarded moments, even the most experienced researcher can fall. The two areas which are most densely saturated with booby traps (if I may change the metaphor momentarily) are 1) the collection of linguistic data from informants and 2) the interpretation of that data. Wittingly or unwittingly, even the most honest and straightforward informant occasionally supplies the researcher with material which is inexact or even fraudulent. An extreme and amusing example of mis-information supplied by a cleverly malicious informant was brought to my attention several years ago.

A certain linguist wished to study a gypsy dialect spoken near his home in Europe. Unfortunately the members of this speech community were hostile and disinclined to divulge any information regarding their language, a tongue of which this man was completely ignorant. Finally, however, a vivacious, intelligent young woman was persuaded to serve as his informant. With her assistance, the linguist recorded in phonetic transcription a list of words and expressions of the gypsy dialect. To his chagrin, he later discovered that the "basic words and common expressions" which he had so painstakingly recorded and so laboriously memorized consisted largely of rather powerful profanity and downright shocking obscenties. The word material was probably sufficient to permit the linguist to work out the phonemic inventory of the dialect, but any attempt to publish this material would certainly have landed him in jail on a morals charge.

Although none of the various informants I have used in my own linguistic fieldwork have even distantly approached the cunning duplicity of this gypsy woman, several have tried from one motive or another (and not always unsuccessfully) to lead me from the straight and narrow path of linguistic correctness. A few typical examples will suffice to illustrate the kinds of pitfalls which are so frequently dug for the linguist by his informants.

In connection with a linguistic and folkloristic problem, I was once interested in determining the geographic distribution of the two chief variant forms of the PaG* word for breath: o:dem / oxdem (StG "Atem," older form "Odem"). As my chief informant for a certain area, I chose a man of exceptional intelligence and unquestionable integrity, who had had splendid training in the humanities and the social sciences. He informed me that the only "correct" word for breath in the dialect was ofdem (which is, actually, a very rare form, being a cross between o:dem and oxdem with the uncharacteristic substitution of the labiodental for the velar voiceless continuant). This man admitted that the people of a neighboring community used other words for "breath"; but they spoke such an outlandish dialect -- he assured me in all earnestness -- that he could hardly understand a word they said. Now the most striking feature of the PaG dialect is its homogeneity. Regional differences in

*Abbreviations and phonetic transcription employed here, are explained at the end of this paper (page 12).

vocabulary, phonology, and morphology do exist; but they are too insignificant to cause even the slightest difficulty in communication. The linguistic features of the neighboring dialect which seemed so "outlandish" to my informant were limited almost entirely to difference in intonation pattern and tempo of speech.

At about the same time I was working on the problem of the geographic distribution of o:dem / oxdem, I was trying also to determine whether the degree of nasalization in words like be:, Sde:, Se: represented primarily a regional, generational, personal or syntactic phenomenon. One of my informants who had usually been co-operative and absolutely reliable, supplied the forms bain, Sdain, and Sain when asked for the dialect equivalents of "leg," "stone," and "beautiful." "Bein" and "Stein" are, of course, the correct StG congeners of the PaG forms be: and Sde:. Sain however, is a hypercorrect nonce formation based on the incorrect analogy: Sde: is to Sdain as Se: is to Sain. The radical vowel e: in the first two words represents a monopthongization of ei resulting from Gmc ai through i-umlaut. In the third word, however, PaG e: represents an unrounding of German ø: which goes back to Gmc o: (OHG Sko:ni, MHG Sø:ne, StG Sø:n, PaG Se:).

More recently I have begun to make a survey of the German dialects spoken in and around Lincoln, Nebraska. Included in the preliminary questionnaire is the sentence "We speak German at home," which is to be translated by the informant into his native speech. On the basis of this sentence alone it is often possible to determine the general dialect of the speaker. In PaG, for example, the translation is: mi:er Swedse daidS dahe:m. In one of the dialects of the "Deutschrusslaender" (Germans from Russia), the sentence is as follows: mi:er blaudere daidS daha:m. Both of these dialects can be designated as colonial languages which are predominantly Franconian in their essential linguistic features. One of my Lincoln informants, however translated this sentence not into the dialect, but into StG: "Wir sprechen Deutsch zu Hause." This informant had developed an attitude of hostility toward his native dialect, which he regarded as a form of "Low" German as opposed to "High" German. Since in his mind only "High" German was "correct," he chose to "forget" the "vulgar" speech of his childhood. This sort of person might be a good subject for the psychologist who is interested in studying the background-erasure complex, but he is frustrating and absolutely useless to the linguist who is trying to investigate scientifically the sounds, forms, and words of his dialect.

Last summer I asked a woman in Iceland why the Icelanders spell the genitive case of Bjørn with the letter a, but pronounce it á. She assured me that it was pronounced exactly as it was spelled, namely with the sound of a for the radical vowel, and illustrated this sound by three times carefully pronouncing the word bjárnar. Although she clearly distinguished the sounds á and a in other languages (she speaks English, Danish, French and German fluently and has a reading knowledge of Norwegian, Swedish, and Latin), she was completely unaware of the marked difference in vowel coloring between the two a's in bjárnar (spelled "Bjarnar"). A foreigner with no knowledge of Icelandic would probably consider á a variant of the phoneme e, whereas it is actually an allo-

phone of a.

Americans, who are accustomed to characterizing German (and, indeed, many other foreign languages) as "guttural," are startled when they hear Germans, Icelanders, and Frenchmen complain that American English is more difficult to understand than British English because "Americans speak way back in their throats." The relaxed manner of speech of most Americans and especially the retroflex l and r cause non-Americans to consider the American language "guttural," while the American who is not phonetically trained misinterprets the many palatal consonants (S, c) and the rounded vowels (ø, ø:, y, y:) of German as velar sounds.

It is obvious from the preceding discussion that one must observe the utmost caution in selecting informants and have infinite patience in eliciting and interpreting information from them. Hostility, or any other negative attitude on the part of the informant toward the researcher, makes successful co-operation in the sometimes tedious job of completing a questionnaire difficult if not impossible. The attitude of the native speaker toward himself and his mother tongue is also extremely important. A feeling of embarrassment and inferiority because of his foreign accent in English or because he speaks a socially inferior dialect will nearly always color his responses. Quite as difficult to deal with as the sullen or reluctant informant is the self-styled "expert," whose excessive pride in his dialect and his mastery of it sometimes impel him to invent words and forms and to volunteer interesting misinformation about his own and neighboring dialects. It is a good rule to take all statements by non-linguists about language or languages cum grano salis. With patience, tact, and friendliness, however, one can usually establish rapport with a sufficiently large number of reliable speakers to secure the linguistic material desired.

Even if the linguist avoids or escapes with only minor injuries from the many pitfalls set by his informants, he must be constantly on his guard against the minefields which surround the area of the interpretation of his data. The intriguing problem of trying to determine the home dialect or dialects of a colonial language, for example, is infested with booby traps. It has become almost axiomatic that a colonial language which is several centuries old did not stem from that area of the homeland with which it now has the greatest number of linguistic features in common. Indeed, it is often impossible to determine the provenience of a colonial language on purely linguistic evidence. This is especially true when the colonial language is a composite dialect.

The dialect geography of colonial languages too has its pitfalls. Supposedly geographical variants frequently prove upon closer study not to be regionally restricted expressions but semantic, syntactic, or even generational and personal differences. This is especially true in transition areas, where words belonging to two different dialects exist side by side with semantic or other differentiation. The word for meadow in the northeastern part of the PaG speech area is Swam. In the southwest it is wis. In the north central area, however, wis means "meadow" while Swam designates a low-lying, wet meadow or swamp. Similarly the form for the modal auxiliary verb wollen "to want" is wele in the southwestern and wole

in the northeastern area. In the transitional zone, however, both forms are used in the subjunctive mood with difference in meaning: ic wod ic hed means "I wish I had" (StG "ich wollte ich haette"), but sel wed ic ned means "I would not want that" (StG "das wollte ich nicht"). In other words the simple wish is expressed by the subjunctive of wole, whereas a wish in a hypothetical situation is expressed by the subjunctive of wele. The southwestern form bode and the northeastern form bodem (StG Boden "floor") appear in the transitional zone as syntactic variants: bode occurs in the nominative and accusative; but in the dative case, especially in the prepositional phrase ufem bodem (StG auf dem Boden "on the floor"), the form bodem is nearly always used. Significant generational differences are the use of e:gne (discussed below) and the pronunciation of the loanword "pie" as boi by the older generation and pai by younger people. Both of these examples of generational differenciation, of course, can be explained on the basis of interlingual influence. But where, after all, can one study a given tongue, so to speak, in a vacuum? "Foreign" influences are never wholly absent; they merely differ in variety and degree from language to language or from speaker to speaker. On the whole, the generational and personal differences in linguistic usage seem to be more marked the stronger and more varied the interlingual influences are. They are especially noticeable on the one hand in composite colonial dialects like those of the "Deutschrusslaender," who have also been exposed to pressure from both Russian and English as well as from Standard German, and on the other hand in such native Indian languages as Omaha, where the influence of English and the radically changed and rapidly changing cultural environment have not only profoundly influenced the native vocabulary but have also undermined the Sprachgefuehl of younger bilingual speakers. (For considerations of space the intriguing but complicated question of personal differences cannot be considered here).

The problem of the interpretation of linguistic data is obviously greatly complicated whenever there is a possibility of considerable interlingual influence. Even such a relatively simple question as the source of loanwords in a given language or dialect can trip up not only the novice but also the experienced linguist. Thus the learned and eminent lexicographer, Dr. M. M. Mathews, editor of the excellent Dictionary of Americanisms on Historical Principles, (Chicago, 1951) has derived a number of American words from the Dutch, although some of them are demonstrably German in origin while others were evidently borrowed independently from Dutch and German at various times and places in this country. Among these borrowings are such common American words as "bake oven," "bushwhacker," "cruller," and "waffle-iron." (Compare in this connection my articles: "Comments on Some Pennsylvania-German Loanwords in the Dictionary of Americanisms," American Speech XXIX (1954), 45-54, and "Pfaelzische Entlehnungen in der amerikanischen Umgangssprache" in the Festschrift fuer Adolf Bach, Bonn, 1956).

In his linguistically sound and reliable monograph "The Pennsylvania German Dialect Spoken in the Counties of Lehigh and Berks: Phonology and Morphology" (Seattle, 1949) Carroll E. Reed lists bju:ro and bi:ro "chest of drawers" as variants of an English loanword. bju:ro: is, of course, the English word "bureau" phonetically modified to conform to the sound pattern of the dialect. bi:ro:, however, cannot possibly

be explained as a phonetic variant of this word. How could one account for the loss of the palatal glide j and especially the change of u: to i:? i: could only represent a StG i: or an unrounded y:. The latter is the case here. The French loanword bi:ro "writing desk" exists in some dialects of Western German and Alsace. Therefore this word must be regarded as a pre-migration borrowing from French, which in the New World underwent a semantic shift under the influence of English "bureau." This, incidentally, is the only linguistic error I could detect in the entire monograph, which is the most compact treatment of PaG phonology and morphology I know of.

Loan formations are much trickier to deal with than loanwords. Even Bloomfield slipped up on the German word gleichen, which in Europe means "to resemble" but in American has the totally unrelated meaning "to be fond of." Bloomfield explained gleichen "to be fond of" as an independent verb derived from the adjective gleich "resemblant" (Language, p. 462). In the first place, a denominative verb would be weak and not strong, as gleichen "to be fond of" is. In the second place, it is difficult to see how a verb formed from gleich "resemblant" could possibly mean "to be fond of." And finally, gleich in the sense of "resemblant" is not in common use in some of the American-German dialects in which gleichen means only "to like" and never "to resemble." Actually we are dealing here with an interesting case of semantic borrowing based on a confusion of "to be like" and "to like." This semantic shift was furthered 1) by the fact that StG and the various German dialects possess a plethora of words indicating various degrees and kinds of fondness, and 2) by the circumstance that the English expression "I like it" is structurally more simple and direct than "es gefaellt mir," "es ist mir lieb," "es steht mir an," "es sagt mir zu," "ich habe es gern," etc. A parallel case is the verb e:gne, which originally meant "to own" but has come to mean among younger speakers of the dialect "to owe." Here again the semantic shift can be explained partly on the basis of the confusion of two approximately homophonous English words" "to own" and "to owe," and partly by the fact that the English idiom "He owes me ten dollars" is simpler than the German "Er ist mir zehn Dollar Schuldig." (I have discussed the problem of loan formations in the PaG dialect in a series of studies, the last of which is "Die Lehnpraegungen der pennsylvania-deutschen Mundart," published in the Zeitschrift fuer Mundartforschung in February, 1955).

"Apee cake," which is the common designation for a popular breakfast cake in Eastern Pennsylvania, has been variously explained as an abbreviation of "Ann Page Cake" or as a product of the Atlantic and Pacific (A and P) Tea Co. Actually it is a loanblend derived from French pain d'épice. Pain d'épice, which means literally "spice bread" and is rendered by bilingual dictionaries as "gingerbread," is a honey-flavored cake which is especially popular in Dijon. Brought to Germany by the Hugenots, pain d'épice there became e:bikuxe. In this form it was brought to America by these people, who had become linguistically German. In America e:bikuxe was partially translated into "apee cake."

Another interesting loanblend in Pennsylvania English is "pelthy." Regarded superficially, this word seems to be a blend of dialect belsic

and the synonymous English "pithy." This obvious explanation, however, is weakened by the fact that the word "pithy" does not seem to be in common use in the areas where "pelthy" is used. This loanblend can be explained satisfactorily only as a hypercorrect analogical formation. The English word "filthy" has been imported into the PaG dialect with phonemic substitution as filsic. The English phoneme th, which is foreign to the dialect, is replaced by s, and the PaG adjective ending ic is substituted for the English ending i. Thus "pelthy" was created in accordance with the formula filsic is to "filthy" as belsic is to "pelthy."

These few examples, which could, of course, be increased a hundredfold, are characteristic of the pitfalls into which the linguist can fall in the collection and interpretation of his data. Most of them can be avoided if the researcher observes a few precautions. Informants should be carefully chosen and judiciously used. Enough native speakers should be questioned on a sufficiently large number of items to assure the rectification of the most glaring misinformation wittingly or unwittingly supplied by one or the other informant. The linguist should further check his material, wherever possible, by observing unobtrusively the use of the language in normal everyday situations.

Probably the easiest pitfall for the neophyte to fall into in the interpretation of his data is the tendency to draw conclusions prematurely or to generalize on the basis of insufficient evidence. A young American linguist recently wrote in a book review about the "ultimate interpretation of linguistic data." One is reminded of the lament of the physicists in the 1890's that all the basic laws of physics had been discovered, and that henceforth research in the physical sciences would be limited to a refinement of methods and measurements. Prokosch just a few years ago gave a radically new interpretation to the first consonant shift (that part of "Grimm's Law" dealing with the development of the Indo-European stops in Germanic). The uncompromising position of the neo-grammarians in regard to the "Ausnahmslosigkeit der Lautgesetze" ("Phonetic laws admit of no exceptions") was made untenable only through the extensive and intensive study of German dialects during the past few decades. And just recently my teacher, Otto Springer, advanced some stimulating new ideas on the still controversial problem of the relationship of High German to the other West Germanic languages and to Gothic. The "ultimate interpretation of linguistic data" is just as remote as the final solution of all the other problems of human behavior and human society. The infinitely manifold features of linguistic reality cannot be forced into a simple formula. And yet even experienced linguists try to do so. The result is usually a neat abstract formulation which bears little resemblance to the real, living, changing language it inadequately tries to define.

In the interpretation of his data, therefore, the linguistic fieldworker should proceed with the greatest caution. Jacob Grimm, one of the founders of the science of linguistics, spoke of the "Andacht zum Text" ("reverence for the text"). The first job of the linguist is to record the data as exactly, objectively, and completely as possible. He must not be influenced by preconceived ideas of what he "should" find. If his findings disagree with existing theories, the theories must be modified to conform with the facts. The facts must not be distorted to fit the theories.

The linguist may discover that purely descriptive or synchronic techniques are inadequate beyond a certain point in the classification, interpretation, and utilization of his data. Especially in the case of interlingual influence he may have to make use of whatever information historical and comparative linguistics can place at his disposal. Occasionally he will have to resort to the help of the archeologist, the anthropologist, and the historian to clear up puzzling phenomena.

I should like to return to the matter of opportunities for linguistic research. Since the beginner often cannot see the individual trees for the forest, it would be well to enumerate a few specific problems and to suggest methods of attacking them. One important phase of linguistic study is the collection of word material and the preparation of glossaries. Care should be taken to note especially the generational differences in the knowledge and use of words. Frequently, for example, Indian informants comment that certain speakers know "old" words or "speak the old language." A painstaking study of these "old" words and a comparison with the vocabulary of younger speakers will contribute much to our knowledge of how and why native vocabulary disappears -- a problem which is more complex than the uninitiated might suspect. Another interesting problem is the recording of life histories and of folk tales and anecdotes. Such recordings are of interest to the folklorist and ethnologist as well as to the linguist. Frequently they can be published in bilingual form, the native text in phonetic transcription with an interlingual literal translation on one page, and a good English translation with footnotes on the facing page. Material of this kind should be recorded on a high-fidelity tape. The word-for-word translations must usually be prepared with the help of bilingual natives. Omaha and Ponca texts of this kind are now in preparation. The same material can also be analyzed phonemically and grammatically if the researcher has the necessary interest and aptitude. After a linguist has once begun to collect word material and connected texts, interesting problems suggest themselves. Frequently linguistic studies done by other scholars raise questions which he feels impelled to try to answer. The works listed below, which have been very stimulating and informative to me, are not only intrinsically valuable but methodologically exemplary. I therefore strongly urge the beginning linguistic field worker to study them along with the well-known works of Bloomfield and Sapir:

Bloch, Bernhard and George I. Trager

1942 Outline of Linguistic Analysis. Baltimore. (This outline is one of the Special Publications of the Linguistic Society of America).

Haugen, Einar

1950a The Analysis of Linguistic Borrowing, Language XXVI, pp. 210-231.

1950b Problems of Bilingualism, Lingua II, pp. 271-290.

Kurath, Hans

1939-43 Linguistic Atlas of New England, Vols. 1-3. Providence.

1949 A Word Geography of the Eastern United States. Ann Arbor.

Leopold, Werner P.

1952 Bibliography of Child Language. Evanston.

1939-50 Speech Development of a Bilingual Child. Evanston. (4 volumes)

1948 The Study of Child Language and Infant Bilingualism, Word IV, pp. 1-17.

Mathews, Mitford M.

1951 A Dictionary of Americanisms on Historical Principles. 2 Vols. Chicago.

Reed, Carroll E.

1949 The Pennsylvania German Dialect Spoken in the Counties of Lehigh and Berks: Phonology and Morphology. (Introduction in collaboration with Lester W. Seifert). Seattle.

Reed, Carroll E. and Lester W. Seifert

1954 A Linguistic Atlas of Pennsylvania German. Marburg/Lahn.

Roedder, Edwin

1936 Volkssprache und wortschatz des Badischen Frankenlandes: Dargestellt auf Grund der Mundart von Oberschefflenz. (New York) (A publication of the Modern Language Association of America).

Springer, Otto

1943 The Study of the Pennsylvania German Dialect, The Journal of English and Germanic Philology LII, pp. 1-39.

Weinreich, Uriel

1953 Languages in Contact: Findings and Problems. New York. (Publications of the Linguistic Circle of New York. No. 1).

American Council of Learned Societies

1942 Conference on Non-English Speech in the United States. Bulletin No. 34, Washington.

The following abbreviations were used:

Gmc - - - - - - - - - Germanic

MHG - - - - - - - - - Middle High German

OHG - - - - - - - - - Old High German

PaG - - - - - - - - - Pennsylvania German

StG - - - - - - - - - Standard German

The phonetic transcription is a simplified version of the international phonetic alphabet. The stops b, d, g are unaspirated. S represents the sound of "sh" in "shoe," th the sound of "th" in "thin." c represents the unvoiced palatal continuant as in StG "ich" and x the unvoiced velar continuant as in StG "ach." Length of vowel is indicated by the colon. In unaccented position, e represents the shva-sound. y and ø represent the rounded sounds of French "tu" and "peu" or German "Muehle" and "schoen." å stands for the sound of "a" in American-English "hat" of "bath."

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MATHEMATICS AND THE ANTHROPOLOGIST

Robert M. Kozelka

Aside from some fundamental work by Karl Pearson in physical anthropology after the turn of the century, mathematics beyond elementary statistics has been conspicuous in anthropology by its failure to make any significant contributions. It is also worth noting that anthropology, in its turn, has been conspicuous among the social sciences for its failure to ask help from mathematicians. The reason is obvious: Anthropologists study all of one culture, all of one site, all of one language (or at least all they can get) -- they are not interested in drawing a large conclusion from a small sample or a general theorem from a restricted hypothesis. What in the world has a mathematician to offer the anthropologist -- or vice versa?

It is unnecessary for a non-anthropologist to point out that many anthropologists have been drawn, at least since the start of World War II, into relatively large-scale studies. Part of this is due to the fact that only since that time has sufficient money been available, through foundations and/or governments, to provide even a first approximation to a satisfactory budget for such studies. Along with such studies came the realization that drawing any noteworthy conclusions from studies in the large was quite different from drawing respectable conclusions about a tribe of 20 old men, or even about 20 boxes of potsherds. Studies of large-scale populations, such as the one described by Roberts et al (1956), suggested a worthwhile cooperation between at least the mathematical statistician and the anthropologist. Benefits accrued to both sides: to the anthropologist through reduced expenses and increased accuracy, and to the statistician through the acquistion of data on types of (statistical) populations not previously studied; so the statistician is finding his way back into anthropology in quite a different context than that established by Pearson so many years ago.

Looked at in one light, this cooperation was no more than that which the sociologist had been enjoying (the word is used loosely) for some time in similar large-scale studies. Statistics, after all, can be considered as a study of patterns: patterns of growth, patterns of arrangement, patterns of discovery. Insofar as these patterns conform to certain standard statistical distributions, much general information can be drawn -- with measured accuracy -- from rather restricted samples. Only recently, however, the study of patterns has been re-evaluated by the statistician (and other mathematicians as well) and has emerged as the study of mathematical models in general).

At the 1955 meetings of the Nebraska Academy of Sciences, the writer presented a paper entitled "On Some Mathematical Models." The title, apparently, was misleading. He was asked before the talk, by one person, if he had some of the models with him! Possibly a non-euclidean plastic highway and some rubber-sheet automobiles would have enlivened the discussion, but the mathematician who talks of models nowadays is really only looking at another side of the idea of patterns familiar to every anthropologist. Patterns of culture are abstractions, in terms of anthropological ideas, of concrete exhibited behaviors. Mathematical models are "second-order" abstractions -- representations of the patterns of culture as mathematical functions and equations. Two or three illustrations from familiar anthropological and sociological problems will perhaps simultaneously clarify the distinction between mathematical models and other abstractions and at the same time may summarize the functions which a mathematician, consulting in the social

sciences, is prepared to perform.

There are two methods available for relating mathematical models to physical situations, and the difference between the two points up the difficulties and virtues of both kinds. The first method is to choose the model and try to find a physical situation to fit it. This is not as specious as it may seem at first sight. Many problems in the social sciences were profitably resolved only because they happened to fall into the pattern familiarly known as the "normal" distribution. The properties of this symmetrical, bell-shaped distribution are well-known and form the basis of classical statistical theory. The studies in physical anthropology have advanced as far as they have not only because they attracted early attention but because so many measurements of natural aspects follow the normal distribution. Thus, in the studies concerning sizes of children (Anonymous, 1932) makers of ready-to-wear clothing benefit from the fact that statements concerning variation in size can be made within rather accurate limits. This is the chief virture of such a procedure: when one uses a well-known model, this model is probably so completely worked out mathematically that predictions made from it can be made within limits of previously specified accuracy, and an appropriate probability of failure attached to such limits. Such limits may have to be exceedingly small, as in astronomical work, or they may be rather large, as in archeological dating, but the important thing is that they can be set with predetermined probability of error. The fact that such errors can be predetermined indicates the other value of predictive models: They can be tested artificially. Some of the models for learning put forward by Bush and Mosteller (1955) were checked first with a table of random numbers and then corraborated by actual experiments. Such a technique, called the Monte Carlo method, provides a quick and economical way to test many social scientific hypotheses.

The second procedure in relating mathematical models to physical situations is the direct opposite of the first. It consists of picking the situation and trying to fit an appropriate mathematical abstraction. This is understandably more complex, but recent work, stemming from the impetus of von Neumann and Morgenstern (1944) has paved the way for advances in this line similar to the advances in the physical sciences following the development of the calculus. This analogy should not be pressed too hard, but it is not unreasonable to make the generalization that since the problems of social science were quite different from those of physical science, the requisite mathematics would be quite different in the two cases. Recent work in so-called "distribution-free" or "non-parametric" statistics has also facilitated this line of inquiry. These two, game theory and non-parametric statistics are in a sense related, in that the results depend only on assumptions not concerned with the form of the underlying distribution. (See Wilke, 1948).

Many of these newer ideas are especially designed to fill an omission frequently inherent in models designed to fit specific situations, namely that models designed in this way are principally descriptive. To say that descriptive models constitute an omission may require some explanation, especially for anthropologists, for whom description by some abstract system may be the desired end of the problem. If this is the case, then the model may take the form of some chemical models with balls and sticks for molecules and binding forces. A striking analogue in the social sciences is that of small group models, with circles and triangles representing people and various arrows to represent forces of attraction or repulsion. Kinship diagrams use the same ideas; that is, they are essentially geometrical and more or less complicated, and may only be designed to serve as a summary representation of certain generalities.

A good example of the nature of the omission inherent in descriptive models is given in an appendix by the writer to Roberts' "Four Southwestern Men" (in press). Here it was desired to summarize responses to some 8000 items from four informants, each representing a different culture. Each item was classified not only by the dimension of culture but other dimensions as well, with varying numbers of choices for each dimension. The n-dimensional model which suggested itself served only as the framework for the essential idea, that of a single numerical measure for each culture-item. Such a measure was to have appropriate combinatorial properties leading to corresponding summative measures over the various dimensions. The idea proposed, that of a simple weighted average with arbitrary weights assigned empirically by the investigator, may seem rather crude by mathematical standards. As a model for complicated situations, however, it allows enough freedom to adjust to the numerous variables and the result is straightforward and easily understood by the nonmathematician. On the other hand, the actual computations involved, which were not carried out in this paper, are so extensive, although elementary, that general applicability and prediction can only be indicated by theoretical terms. Since the method involved is familiar in many disciplines, the actual computations were not necessary.

The question of applicability of method and resulting model must be solved by the anthropologist and the mathematician working jointly, preferably before the project is initiated. The simple weighted average, convenient for description, may be computationally unsuitable for prediction. The underlying distribution may not be known, or randomness may not be present, in which case no measure of error can be assigned. The effect on the descriptive model of a change in one of the variables, as in the small-group models, may not be known, or in the case of certain regression models, may be overwhelming to compute. Even high-speed computing machines require a considerable amount of time for programming, and for a single experiment (or interview) the use of these machines may not be economically feasiable. It is conceivable that the availability of these machines will be a curse as well as a blessing to many social scientists who have yet to learn that a linear (first-degree) model forms a sufficiently good approximation to a great many situations, especially those in which random or human errors tend to play a large part.

These examples illustrate both the dilemma and the hope of the anthropologist (or any social scientist) who wishes to use mathematical methodology. In handling any reasonable amount of data it is better to use mathematical, especially statistical, techniques and use them slightly incorrectly than to use none at all. That is, conclusions based on the normal distribution may be acceptable even if the population sampled is not quite normal or the sample is not quite random, as long as a warning to this effect follows any general conclusions. On the other hand, no one can allow the model to dictate what is studied, especially in anthropology, and the appropriate technique may not be familiar to the researcher. The social scientist has passed the state where he can generally know enough mathematics and statistices to solve or even approach all of his problems: one course in statistics is no longer sufficient to indicate even all the applications of the theory, as it was in Karl Pearson's time. No longer should he feel, for example, that he has to know all the chemistry necessary to do a C-14 dating; some do, but this is not required to do adequate work even where such dating is part of the problem. The state of mathematically useful techniques is now, with the recent advances in distribution-free methods and sampling schemes, such that no anthropologist should feel it necessary or desireable to master more than a few classical approaches. What he should have is an understanding of the types of problems that the mathematician is prepared to deal with, and what types of solutions may be expected. In essence, enough background is required on both sides so that the anthropologist can communicate with

the mathematician and vice versa. Then let the mathematician do the work.

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University of Nebraska

'QUILL FLATTENERS' OR POTTERY MODELING TOOLS?*

Richard P. Wheeler

Cultural inferences from artifacts recovered by archeological methods depend on the functional identification of the specimens, and these have often been made, in the Americanist field, by reference to present-day counter-parts and practices. Sometimes artifacts of distinctive form and utilitarian purpose, found in sites of historic and late prehistoric date, elude clearcut identity as to function because of the absence or frailty of ethnographic control of the once-accessible data. An example of items of this sort are spatula-shaped objects made of split segments of heavy rib, with polished and rounded or bluntly pointed tips and smoothed straight or irregular sides. Random scratches or, rarely, incised geometric designs appear on the lateral surface, and the cancellous tissue on the medial surface may or may not be worked down.

In two recent archeological reports the suggestion is made that bone implements of this description from sites in the Northern Plains are 'quill flatteners.' In one report, Donald J. Lehmer (1954, p. 67), seeing a similarity in general form between the 89 specimens from the Dodd site in the Oahe Dam area, South Dakota, and a number of specimens in the Chicago Natural History Museum collected among the Pawnee and identified as 'quill planes' by George A. Dorsey, states: "Lacking any better suggestion for the function of the Dodd site pieces, it seems reasonable to assume that they too were used for flattening porcupine quills which were to be used as decorative elements." In the other report, Waldo R. Wedel (1955, pp. 125-126), while likening the highly polished and rounded tips of the ten specimens discovered in as many graves at three aboriginal cemeteries near Mobridge, South Dakota, with those of two more elaborate implements described in the literature, namely, a 'quill-flattener' attributed to the Sioux and a 'bone instrument for making moccasins' from the Fort Clark site in North Dakota, wants it understood that his identification of the Mobridge specimens as 'quill flatteners' "is nothing more than a possibility," since the few examples of porcupine quillwork in the graves "could as well be the results of intertribal trade as of a local art."

In recent accounts of the porcupine quillwork of the western Sioux by Carrie A. Lyford (1940, pp. 41-55) and of the Blackfoot by John C. Ewers (1945, pp. 27-32), the fact is recorded that after the already flattened quills had been sewn down they might be (or were) further smoothed, pressed, flattened by a tool described by Lyford as a "special instrument made of a smooth flat bone" and by Ewers simply as a "bone or horn instrument." In his brief article on porcupine quillwork illustrated by two superbly embroidered Huron Indian moccasins and other examples, B. W. Merwin (1918, p. 51) says: "The only implements used even in the most intricate work were an awl and sometimes a piece of bone to flatten the quills." Whether the archeological specimens I am discussing are analogues of these tools is uncertain inasmuch as necessary details regarding the form, surface appearance, and condition of the latter are not given.

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*Submitted with the permission of the Secretary of the Smithsonian Institution.

This winter, in the course of examining the artifact material excavated in the fall of 1955 under my direction at the Breeden site, 39ST16, in the Oahe Dam area, South Dakota, it occurred to me that the bone implements catalogued as 'quill flatteners' and illustrated here by examples of five recognizable variants (Fig. 3, a-e) may have been modeling tools used in shaping and decorating much of the pottery present in the collection. Points favoring this induction are: (1) the relatively high frequency of the specimens -- next to awls, they are by far the most numerous tools in the worked bone category -- suggesting that they were associated with a locally important industry; (2) the fragmentary condition of the majority of the pieces -- nearly two-thirds of the specimens are end- or mid-sections -- suggesting that they were pressure applying tools; and (3) the angular shape and smooth 'feel' of many of the pottery rim sherds (Fig. 3, h), indicating that they had been trimmed and smoothed by easily manipulated implements, and the close correspondence in many of these sherds between the shapes of the intaglio decorations -- whether a band of horizontal trailed lines on the exterior rim surface or a row of punctates at the outer edge of the lip (Fig. 3, i, j) -- and the shapes of the working ends of the bone tools. The latter observations suggest that the decorative markings were made by such bone tools.

I found, in reading a popular ceramics handbook and in consulting with a teacher of handcrafts at the University of Nebraska, that the bone tools under discussion are similar in form to orange wood sculpting tools -- some of which have a wire knife at one end -- used by sculptors and ceramicists today (Fig. 3, f, g).

To test the several observations mentioned above, I fashioned ten miniature pots out of clay flour tempered with sand or pulverized granite pebbles. Five of them were trimmed, smoothed and decorated with one of the Breeden bone implements, and the rest were similarly treated with a small wood sculpting tool. The specimens in the two groups are indistinguishable in appearance and, allowing for enormous differences in size and skill, resemble the archeological ceramic prototypes.

Thus, on the basis of cognition and experimentation, I would suggest that the bone tools described herein merit consideration as pottery modeling tools used by former residents of the Northern Plains in finishing some of their earthenware vessels. I know of no direct support in the ethnographic literature for such functional identification. However, in the single reference I have seen to the use of pottery tools in the region, Melvin R. Gilmore (1925, p. 287) mentions a wood tool, "like a flat club," used to malleate the clay, and a small pointed and edged wood tool, used in applying "the desired pattern of decoration by incision." Following this, it seems to me entirely possible that if shaped wood tools were used in decorating pottery in the Northern Plains, comparable ones of more durable bone may have been similarly employed there also.

I would appreciate receiving references and comments on this topic from interested readers.

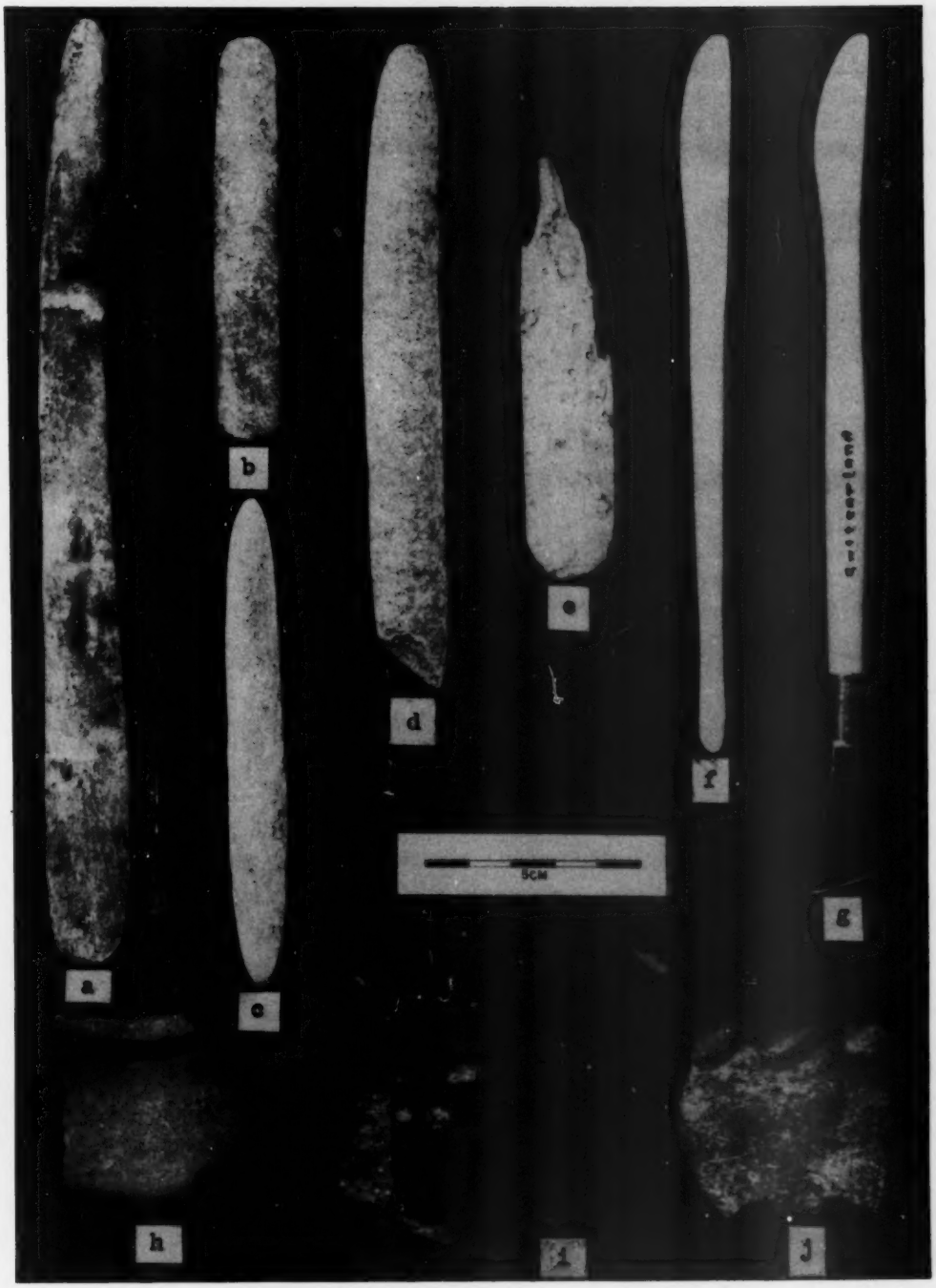

Figure 3.

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Note on the Objects Illustrated in Figure 3.

a - e, Bone tools, each representing a shape-variant, from the Breeden site, 39ST16; f, g, modern orange wood sculpting tools; h, pottery rim sherd of type, Foreman Incised; and j, pottery rim sherd of type, Anderson High Rim, subtype, Punctate. The sherds are from the Breeden site, 39ST16, and represent types and subtypes discovered and named by Lehmer.

Missouri Basin Project
Smithsonian Institution

A WOODLAND SITE NEAR WILLISTON, NORTH DAKOTA

W. Raymond Wood

The purpose of this note is to present full descriptive data on a ceramic site in northwestern North Dakota which has been briefly described by Will and Hecker (1944, pp. 116-117). According to these writers, Site 32MZ2 is situated on a flat-topped promontory about 2.5 miles southwest of the town of Williston, on the south side of the Missouri River, in McKenzie County. The legal description is the NE-1/4 SE-1/4 Sec. 10, T153N, R10W. The promontory is covered with bone to a depth of 16 or 18 inches from the surface, and deep layers of ash, bone, and flint chips are covered with a layer of clay. The artifacts from the site, in the collections of the State Historical Society of North Dakota, include pottery, chipped and ground stone and fragmentary bison bones.

POTTERY

Example A (Fig. 4, H).

Sample: 1 rim sherd
Paste:
Temper: Coarse, angular particles of sand, averaging 1 mm. in diameter.
Texture: Surface is rough, clay is compact, with some tendency to cleave horizontally.
Hardness: 4
Color: Exterior is light to dark gray; interior is black, core is black.
Surface Finish: Horizontal cord roughening occurs on the rim exterior. The cords used have an "S" twist, and are 1.5 mm. in diameter. Fine, vertical cord roughening is on the rim interior.
Decoration: An externally applied deep punctate with a rounded bottom is 25 mm. from the lip. It is 10 mm. wide and 7 mm. deep, and creates a node on the sherd interior.
Form:
Lip: Beveled to exterior, and down at about a 45 degree angle. Some protrusion results from cord roughening applied diagonally to lip.
Rim: Straight. Thickness: 5 mm. (lower rim); 10 mm. (lip).

Example B (Fig. 4, F, G).

Sample: 2 sherds from one vessel.
Paste:
Temper: Small particles of sand, averaging 0.5 mm. in diameter.
Texture: Surface is rough, clay is compact, with breakage in straight lines with crumbly edges.
Hardness: 4
Color: Exterior is light buff or gray; interior and core, black.
Surface Finish: The rims are vertically cord roughened, and some cord roughening occurs on the lip as well. The coarse cord used on the exterior is so superimposed that the twist of the cord is not distinguishable.
Decoration: Application of a triangular tool wrapped with a flat, narrow, material to the inner rim resulted in impressions that are V-shaped in cross section. The impressions are deepest near the lip, and decrease in depth as distance from the rim increases.

Form:

Lip: Flat to rounded, with some beveling on the exterior. Some protrusion results from cord roughening applied to the lip.

Rim: Excurved.

Neck: Somewhat constricted, and rim joins shoulder in a smooth curve. thickness: 5 mm. (Lower rim, neck); 8 mm. (lip).

Body Sherds

Sample: 21 sherds

Surface Finish: All sherds are externally cord roughened, with either parallel or random order. The cords used were large and fibrous, and the twist is not distinguishable. The sherd interiors are smooth, with some striations and finger impressions visible. The exterior cord impressions average 1.5 mm. in width.

Thickness: range, 6 to 9 mm., mean, 8 mm.

Color: Exterior is light buff to brown, with some blackening resulting from carbonized material. The interior and cores are black.

Hardness: 3.5 to 4.

WORK IN STONE

A small ground, granite object is broken, but is elliptical in outline and hemispherical in cross section. The flat side is pecked and the convex side is polished. The original length is not determinable. Thickness, 19 mm.; width, 26 mm. (Fig. 4, J).

A variety of small endscrapers occur, made from a local brown chalcedony, "Knife River Flint," and gray and reddish flint. The working edges are steep, and the upper side bears only primary flake scars. They are small, varying from 16 to 35 mm. long, and between 15 and 20 mm. wide (Fig. 4, D, E).

Projectile points of two forms occur. One of these is triangular, with straight to convex bases and slightly convex sides. Side notches occur near the base (Fig. 4, A, B). On two broken specimens the length exceeds 35 and 45 mm.; width is 22 to 23 mm. The bases are smooth, and cross sections and longitudinal sections are lenticular. The other form (Fig. 4, C), suggested by two badly broken specimens, may have corner notches. Each specimen recovered is fragmentary, but in each instance the weight exceeds 5 grams.

DISCUSSION

The sherds from 32MZ2 closely resemble specimens from certain sites in Manitoba, and also specimens from some sites in Nebraska. Internal nodes occur on cord roughened rims from the Stott site in Manitoba, which Vickers assigns to the Manitoba Focus of the Headwaters Lake Aspect (Vickers, 1949, pp. 32-34, Pl. VII, upper right). This aspect is tentatively identified as the cultural remains of the Assiniboin Indians (Wilford, 1945, pp. 312-329: MacNeish, 1954) and 32MZ2 may possibly be identified as an Assiniboin site. The pottery from 32MZ2 also resembles certain Woodland pottery from Nebraska designated as Valley Cord Roughened (Kivett, 1949, pp. 67-69, Pl. V, lower left, Pl, VII, lower right). This pottery is characteristic of the Valley Focus, defined on the basis of excavation of site 25VY1 in central Nebraska, (Hill and Kivett, 1940).

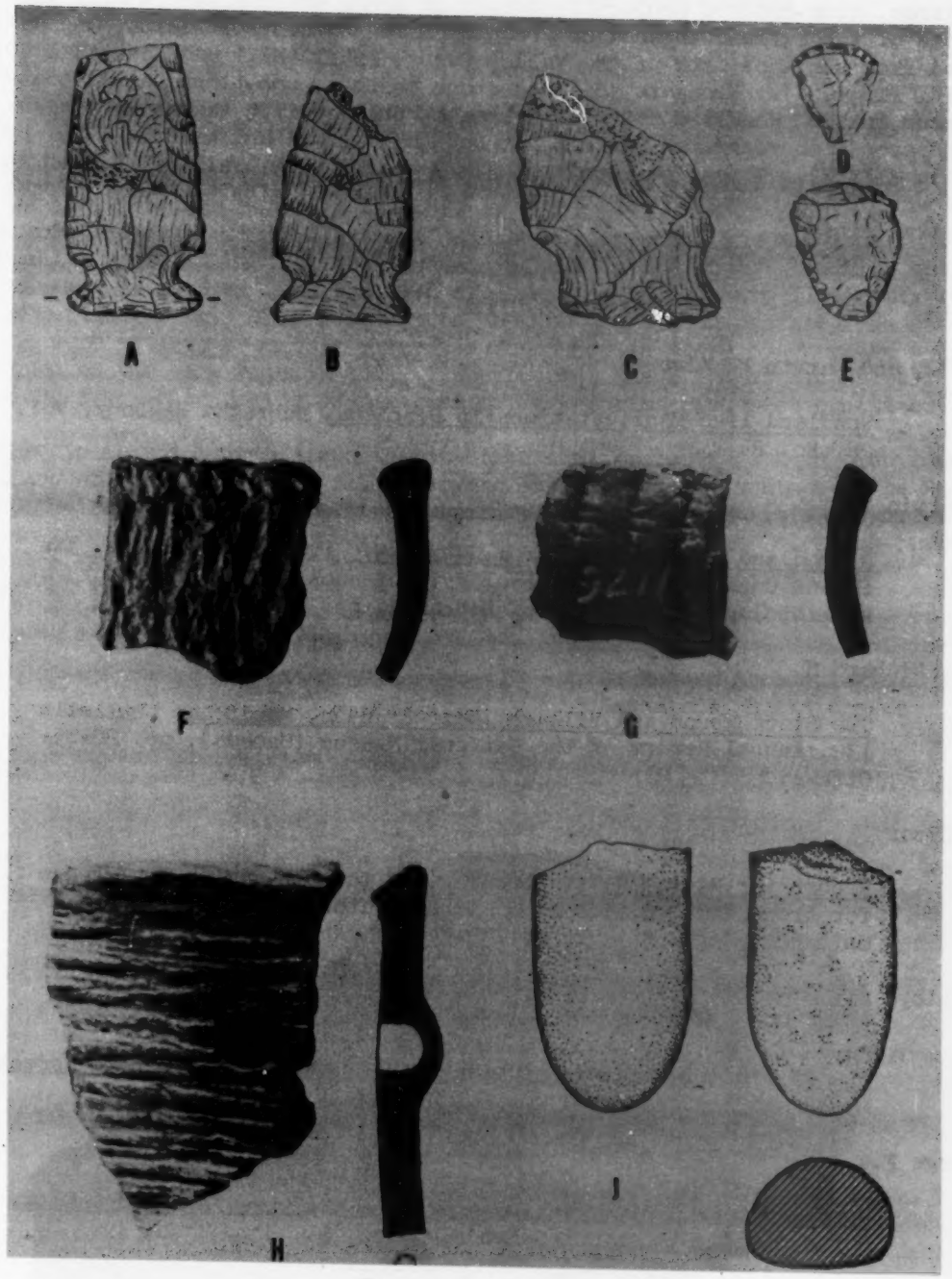

Figure 4.

The large, side-notched projectile points at 32MZ2 are similar in size and form to some of those found in Woodland complexes in the Central Plains, and are illustrated from the type site of the Valley Focus, 25VY1 (Hill and Kivett, 1940, pp. 168-170, Pl. V, 1). Elongate polished stone objects, sometimes called boat-stones, occur in some Woodland sites in Nebraska, but specific taxonomic assignment of the specimens recovered has not yet been made (Hill and Kivett, 1940, pp. 218-219, Pl. XIX, 2). It may be seen that 32MZ2 shares some traits with both the Manitoba Focus of the Headwaters Lake Aspect in Canada and with the Valley Focus in the Central Plains.

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University of Nebraska

THE SLEEPING BUFFALO, A STONE EFFIGY FROM SASKATCHEWAN

H. K. Cronk

This remarkable carving was found near Saskatoon, Saskatchewan about 1945. It was picked up after being dislodged from the bottom of a furrow while breaking an old slough bottom. The ten inches of soil which had covered it, suggest some degree of antiquity and possibly the work of the Indian. It is fashioned from cream colored quartz, smoothed but not polished. It weighs six and a half pounds, is nine and a half inches long and five and a half inches high. The high quality of workmanship is evident when the eyes are examined as they are distinctly set out by a definite ring. This ring can be seen on the eye in profile. No other artifacts were noticed when this piece was found nor is there any surface evidence to be found on the same section today. The nearest known Indian camp site is three miles away where sherds of nineteen different rims have been excavated.

There is an unconfirmed report that other similar carvings have been found in "the Valley of the Sleeping Buffalo" between Saco and Malta, Montana. The Saskatoon Archeological Society has endeavored to verify this report but it's enquiries remain unanswered. However, a member of the Society saw a large "sleeping buffalo" about three feet long in front of the American Legion Health Plunge located at Warm Springs in the Valley about 22 miles east of Malta.

Saskatoon Archaeological Society
Saskatoon, Saskatchewan

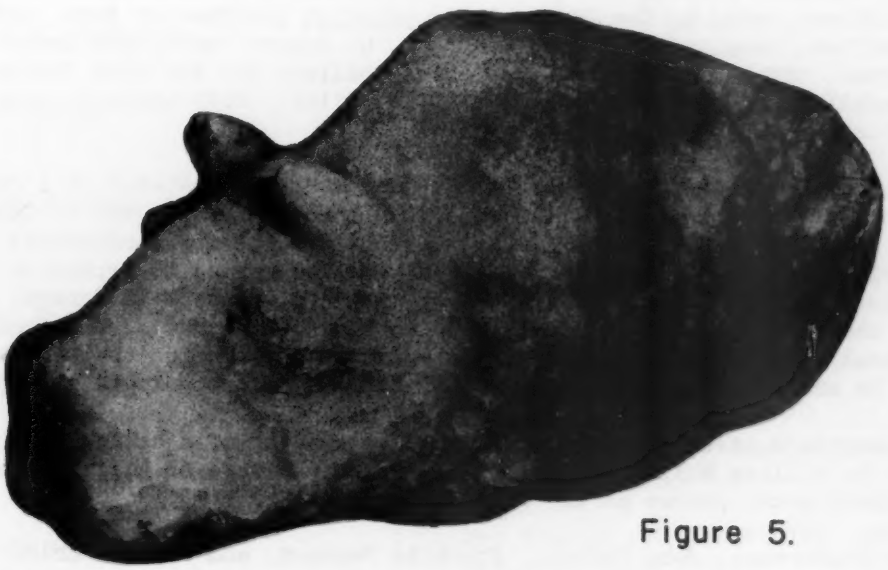


Figure 5.

THE THIRTEENTH PLAINS CONFERENCE FOR ARCHEOLOGY -- 1955

Donald J. Lehmer

The 13th Plains Conference met on Thursday, Friday and Saturday, November 24 - 26 in the traditional setting -- the campus of the University of Nebraska in Lincoln. Thursday morning was given over to registration, inspection of exhibits, and conversation around the "coffee" machine in the corridor adjoining the Laboratory of Anthropology. After lunch, the formal meetings began with reports on 1955 field work in western Iowa, Missouri, Nebraska, and North and South Dakota. Thursday evening there was a cocktail party at the Cornhusker Hotel followed by Thanksgiving dinner in the Georgian Room.

On Friday morning material of considerable interest to all members of the conference was presented in a panel discussion of the recent finds of human skeletal material near Turin, Iowa. The proceedings were initiated with a paper by M. K. Elias on "Vegetation of the Plains Before the Advent of Man," after which the Turin finds were described by W. D. Frankforter and Reynold J. Ruppe, Jr. and commented on by E. Mott Davis, Franklin Fenenga, and H. M. Wormington.

Friday afternoon's formal papers are listed below. Dr. Wormington's summary of the work she and Dr. Mulloy had done in Alberta in 1955 was of particular interest because of the information provided on the little-known Canadian section of the Northern Plains. Friday evening a session was held in the auditorium of the Nebraska State Historical Society. It consisted of a business meeting and an illustrated talk by Dr. Wesley R. Hurt, Jr. on his recent work in South America. Later, conference members were guests of the Nebraska Historical Society for coffee in the Museum staff room.

During the business meeting the financial report was presented and accepted, the sum of $50.00 was voted to the Plains Anthropologist publication fund, and the nominating committee, consisting of Paul Beaubien, G. Hubert Smith, and Henry Hamilton, Chairman, offered Richard P. Wheeler as chairman for the 14th Plains Conference and Franklin Fenenga to succeed himself as editor. Both nominees were elected unanimously

The final meeting of the Conference on Saturday morning consisted of a round-table discussion of "The Concept of the Plains in Anthropology" by John L. Champe, E. Mott Davis, Preston Holder, Marvin F. Kivett, Donald J. Lehmer (moderator), Richard P. Wheeler, and H. M. Wormington. The discussion served to emphasize the fact that, while the 'Plains Culture Area' may be a useful concept in general studies and courses at the introductory level, there is an enormous amount of both temporal and areal cultural variation within the region -- so much variation that any serious treatment must be made in terms of individual tribes and/or archeological complexes.

Saturday morning's session was enlivened by the reading of a cablegram to the Conference sent by William Mulloy and Carlyle S. Smith who were on Easter Island instead of in their usual places at the meetings.

During the Conference, John Corbett, Franklin Fenenga, and Preston Holder served as chairman for individual sessions. The very important preliminary arrangements were made by Richard P. Wheeler as Program Chairman and John L. Champe, Local Arrangements. To all five the sincere appreciation of the Conference and of the Conference Chairman.

Reports and Papers Presented to the 13th Plains Conference

Thursday, November 24.

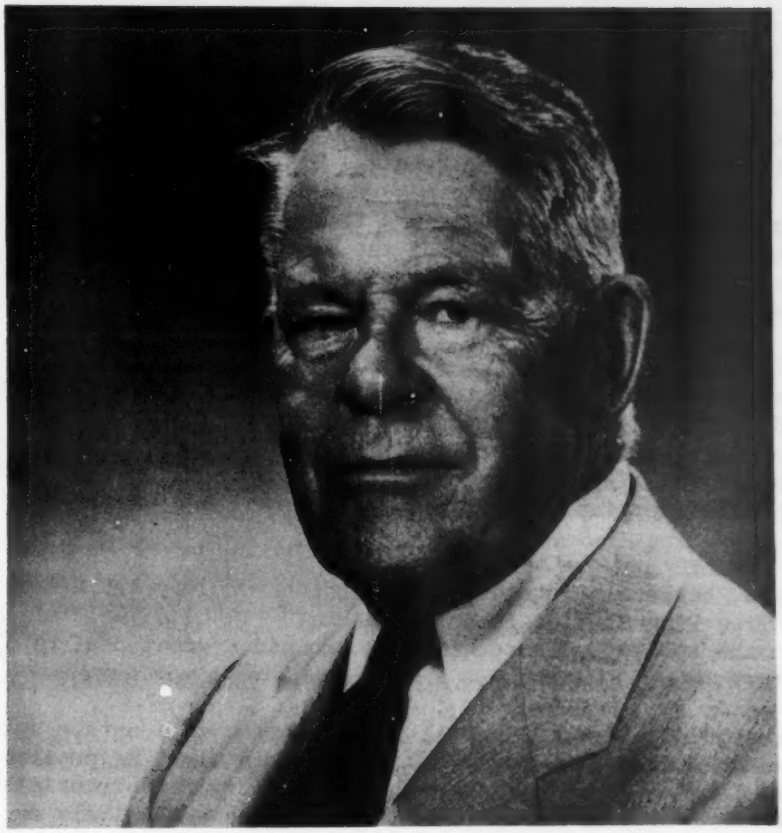
Reynold J. Ruppe, Jr.	Excavation of a Site on Mill Creek, Cherokee County, Iowa.
Marvin F. Kivett	Excavations at the Crow Creek Site, 39BF11, and at site 39BF4, Fort Randall Reservoir area, S.D.
Roger T. Grange, Jr.	Salvage work at site 39BF204, Fort Randall Reservoir area, S. D., by the University of Kansas.
Alfred Johnson	An Intensive Survey of the Skunk Island sites, Fort Randall Reservoir area, S. D., by the University of Kansas.
Richard P. Wheeler	Excavations in the Oahe Dam Area, South Dakota.
Wesley R. Hurt, Jr.	The Swan Creek Site, 39WW7, Oahe Reservoir area, South Dakota.
Alan R. Woolworth	Excavations in Sioux County, North Dakota.
Robert T. Bray	Excavations in the Table Rock Reservoir area, Mo.
J. Mett Shippee	A cave in Calloway County, Missouri.
Richard A. Marshall	Excavations in Lander's Shelter 2, 23SN245, Stone County, Missouri.
E. Mott Davis	Archeological Survey in the Nebraska Panhandle.
Franklin Fenenga	Recent Work of the University of Nebraska State Museum.

Friday, November 25.

W. Raymond Wood	Perforated Elk Teeth on Plains Indian Costumes.
James H. Howard	The Mescal Cult of the Southern Plains, a precursor of Peyote.
David B. Stout	The Concept of 'The Plains' and the Human Relations Area Files.
Malcolm J. Arth	Recent Field Work among the Omaha.

Registrants at the Thirteenth Plains Conference

Malcolm J. Arth	University of Nebraska
Paul L. Beaubien	National Park Service, Omaha
Clement Blakeslee	University of Kansas
Robert G. Bowman	University of Nebraska
Dr. & Mrs. John L. Champe	University of Nebraska
John M. Corbett	National Park Service, Washington, D.C.
Donald D. Davis	Glenwood, Iowa
E. Mott Davis	University of Nebraska
Kenneth Echternacht	University of Kansas
Maxim K. Elias	University of Nebraska
Charles E. Eyman	University of Wichita
Franklin Fenenga	University of Nebraska
Richard G. Forbis	Vernal, Utah
W. D. Frankforter	Sanford Museum, Cherokee, Iowa
Roger T. Grange, Jr.	Nebraska State Historical Society
Mr. & Mrs. Henry W. Hamilton	Missouri Archeological Society
Joyce Wike Holder	Washington University
Preston Holder	Washington University
David Horr	University of Kansas
James H. Howard	Kansas City Museum
Carl G. Humphrey	Mullen, Nebraska
Wesley R. Hurt	University of South Dakota
John C. Ives	University of Iowa
Mr. & Mrs. Harold Janzen	University of Nebraska
Alfred Johnson	University of Kansas
Richard O. Keslin	University of Missouri
Marvin Kivett	Nebraska State Historical Society
Dr. & Mrs. Donald J. Lehmer	University of Omaha
Lee G. Madison	River Basin Surveys, Lincoln
Richard A. Marshall	University of Missouri
Merrill Mattes	National Park Service, Omaha
Louis H. Powell	The Science Museum, St. Paul
Ray Price	Nebraska State Historical Society
Will G. Robinson	South Dakota State Historical Society
Paul R. Rowe	Glenwood, Iowa
Reynold J. Ruppé	University of Iowa
J. Mett Shippee	North Kansas City, Missouri
G. H. Smith	River Basin Surveys, Lincoln
Ted Steegman	University of Kansas
Alice Ann Stofer	Barnard College
D. B. Stout	University of Iowa
T. M. Stout	University of Nebraska
David Thompson	University of Kansas
Mr. & Mrs. R. P. Wheeler	River Basin Surveys, Lincoln
Lloyd A. Wilford	University of Minnesota
C. Wodder Niels	University of Nebraska
W. Raymond Wood	University of Nebraska
Alan R. Woolworth	North Dakota State Historical Society
H. M. Wormington	Denver Museum of Natural History



GEORGE F. WILL

George Francis Will was born at Bismarck, Dakota Territory, on November 8, 1884 and died in that city on October 2, 1955. His father Oscar H. Will, was a pioneer nurseryman in Dakota Territory and did much to utilize the agricultural products, such as corn, which had been developed by the Mandan, Hidatsa, and Arikara tribes. In later years, this became one of his son's major interests.

George Will received his early schooling at Bismarck and went on to Harvard University in 1903, graduating in the class of 1907. From his graduation until his death he was at the Oscar H. Will Company, serving for many years as president of the firm. He was a member of the Bismarck School Board for more than 30 years and held offices in a wide variety of other civic organizations. Of more interest to anthropologists is the fact that he was active as a Director of the North Dakota State Historical Society from 1923 to 1955; serving as president of the Board of Directors from 1942 to 1955.

He was the dean of Plains Archeologists for years as he did his first excavation at the Bourgeois or Double Ditch site in company with Herbert J. Spinden in the summer of 1905. This was probably the first formal excavation in the state of North Dakota. The results of this season's work were published in 1906 by the Peabody Museum as "The Mandans: a Study of Their Culture, Archaeology, and

Language." This publication has remained a basic reference work on the Mandan and at the present is the only comprehensive report yet published on a site in North Dakota.

During the years from 1905 to 1945, he did a great amount of site survey work in this region and published a number of papers on the results. One of the more important accomplishments of this survey work was his paper, "Archaeology of the Missouri Valley," published by the American Museum of Natural History in 1924. In this paper, he summed up his own findings, and those of earlier colleagues in relation to sites in this state. There, many of the Indian villages were correlated with historical references for the first time. In 1933, he published, "A Resume of North Dakota Archeology," which summed up the investigations and conclusions to that date.

He is perhaps best known amongst present day archeologists as the senior author of "Upper Missouri River Valley Aboriginal Culture in North Dakota" published in 1944, and written in collaboration with Thad C. Hecker. This paper stated his beliefs on the archeological sequences in North Dakota along the Missouri River, and gave a site-by-site appraisal and classification of these earth lodge village sites.

In recent years, he became interested in dendrochronology and after a long study, published "Tree Ring Studies in North Dakota," (1946) as well as a number of other papers on the subject. These writings aroused a great deal of interest and some controversy.

In Plains Anthropology, George F. Will is noteworthy as a native son whose span of activity reached from 1905 to 1955. During this time, he published some 20 papers on the archeology of the area, several on ethnology, and translated (for the first time) an important military journal. He also wrote extensively on various phases of history in the Northern Great Plains, as well as natural history. His contributions in these various fields have added much to the understanding of this region and will remain as basic sources.

His achievements in archeology were not merely local. He served as Treasurer for the Society for American Archeology and acted as a consultant on committees concerned with archeology on a national scale.

In 1940, the North Dakota Agricultural College awarded him an honorary degree of Doctor of Science in recognition of his research and writing in seed and plant development and anthropology.

Those of us who knew George Will personally and visited with him at Bismarck, remember him as a generous and congenial host who could discuss problems with the specialists, but who was always willing to listen to the beginners.

Alan R. Woolworth
North Dakota Historical Society
Bismarck, North Dakota

Archeological Investigations in the Oahe Dam Area, South Dakota, 1950-51. Donald J. Lehmer. Smithsonian Institution, Bureau of American Ethnology, Bulletin 158 (River Basin Surveys Papers No. 7). 1954. xi 190 pp., 22 plates. Superintendent of Documents, U. S. G. P. O., $1.50.

Lehmer's publication deals with the investigation of two sites on the Missouri River in central South Dakota. Field work was conducted under salvage conditions, and the impressive amount of information recovered testifies to the efficiency of the River Basin Surveys and the cooperative attitude of the Corps of Engineers. One of the sites (Dodd) contained three components with some superposition of component elements. The other site (Phillips Ranch) was a single component unit situated about a mile from the Dodd site.

Since the primary function of the River Basin Surveys is the preservation of data threatened by construction activities, it might well be argued that a description of the position and empirical characteristics of the objects and features found would constitute a reasonable goal for the Survey's publications. Lehmer's paper attains this goal and accomplishes a great deal more. It is an exercise in archeological methodology characterized by a fresh approach to problems in association and classification, and the techniques used are worthy of serious discussion. I intend to emphasize this aspect of the publication for two reasons. The first is that most of the readers of the PLAINS ANTHROPOLOGIST are probably more competent to judge the substantive results than I am. The second reason is that the statistical and other methodological innovations have important implications for archeology in general, but there is no well-established background of theory and practice against which to judge them. This being the case, every man must be his own expert. At our present stage of development there is no possibility of "proving" that Lehmer's techniques are right or wrong, and even the question of whether they are better or worse than competing methods for attaining a specified objective is a difficult one. It is certain, however, that such techniques are needed and that the final test of what is best must come from the pooled judgment and experience of archeologists. Lehmer has made a distinct contribution to this process whether or not any one of the specific procedures he uses survives as a generally recommended method.

The occurrence of three distinct house patterns, sometimes superimposed, suggested that three successive components were represented at the Dodd site, and Lehmer was faced with the problems of deciding whether there was a significant difference in the artifacts associated with each pattern, of assigning individual findspots to the proper component, and finally of assigning each of the various types of artifacts to its component. In the first case, the comparison was made on the basis of sherd counts of pottery types, but in the second case it was necessary to operate with surface treatment of body and neck sherds because of the few rim sherds at the individual findspots. The two different bases of classification presumably make the two comparisons largely independent and thus constitute a check on the validity of the three foci.

The technique used for demonstrating that the artifacts associated with the three kinds of house patterns differed significantly consisted of a comparison of the pottery found on the house floors with the total sample of pottery from the site. More specifically, the standard error of the difference for the proportion of each pottery type from a particular house class and from the total pottery collection from the site was computed. In the usual application of this statistic, the actual difference between the proportions is divided by the standard error of the difference; if the difference is three or more standard errors it is regarded as

significant in most situations because there is something less than 0.5 percent chance of as large a difference arising from sampling vagaries alone in a normal distribution. Lehmer's problem was not the comparison of the individual pottery types, however, but was rather an over-all measure of the difference between the pottery of the three classes of houses. To solve this problem, he summed the individual pottery differences expressed in standard errors and divided the total by the number of pottery types to get a mean difference. Taken at face value, this looks like a reasonable procedure -- the mean is the best single value if it is necessary to replace a series of values by a single value. But what this average of differences in units of standard error signifies in terms of probability is not clear to the reviewer. The mean differences for the three components range from 10.2 to 22.0, which certainly suggests that the chance of the three pottery samples having been drawn randomly from a common parent population is infinitesimally small. Lehmer indicates that a mean difference of 5 standard errors represents a critical significance level for these data, the particular figure having been suggested by the distribution of the samples from all findspots. He also argues (Footnote 5, p. 38) that the level of significance should be raised from the customary three standard errors on the ground that "The individual loci at the Dodd site appear to represent three different cultural complexes. Because of this, samples drawn from them must be considered to be products of purposive rather than simple sampling." But "purposive" and "simple" refer to the manner in which the sample is drawn, not to the nature of the parent population (or populations) from which the drawing is made, and this line of reasoning needs further clarification. Nevertheless, I agree that the orthodox significance levels should be revised for archeological sherd samples, but the primary reason this is so is that the samples are not random in most cases because sherds from individual pots tend to occur together. However all this may be, it is certainly clear that the difference in pottery among the three components can hardly be ascribed to the vageries of random sampling from a homogenous population. There is no reason to quarrel with Lehmer's conclusion.

This spiritless criticism brings me to what seems to be the major weakness of Lehmer's work, his tendency to devise original and slightly off-color techniques when quite adequate methods are ready at hand. The comparison of the pottery from the house floors is a classic case for the heterogeneity chi-square, a technique in every respect more informative than the mean standard error for this purpose because of its nonparametric character and its consideration of the entire distribution.

In the second application of the mean standard error, the assignment of findspots to one or another of the three foci, the situation is less clear. Lehmer's bar graphs (Fig. 36) suggest two kinds of findspots, those falling within 5 mean standard errors of the pooled body surface treatments of the pottery from house floors assigned to a particular focus and those differing by more than 5 standard errors from this average value. Laying aside a troublesome problem in independence, the exposition of this matter would have been improved by furnishing the reader the mean standard errors of each findspot with respect to each of the three foci, rather than only to the one with which it showed the closest relationship. Thus we are told that the pottery from the floor of Feature 15 differs by no more than 4.9 mean standard errors from the pooled Stanley Focus distribution, but we are not told by how much it differs from the Anderson and Monroe foci. Since Lehmer has abandoned the probability implications of the standard error, it would seem that a simple coefficient of likeness calculated from the percentage distributions would have served equally well here. Probably the most elegant technique for this purpose would have employed discriminant functions, but I cannot blame anyone for avoiding so formidable a method.

A final statistical device to be discussed is the index of association used to assign artifact types to the proper focus. I will again only mention that there

is a difficult problem of independence in the case of the pottery attributes which help to define the foci. Lehmer's approach to the problems of association again ignores the generally recommended procedures and instead compares a weighted number of occurrences of a given category within the focus with a similary weighted number of occurrences outside of the focus. The index attempts to consider both the relative number of occurrences within and not within the findspots of a focus and the distribution of the occurrences over these findspots, a situation which can result in serious confusion. This can be illustrated with the following example, where the problem is to test the association of a particular trait and a particular focus. A certain trait occurs in every findspot of a particular focus, but each occurrence is of one specimen only. The same trait occurs abundantly in every other findspot on the site. Lehmer's index would be computed as:

$$\frac{(a \times b) - (c \times d)}{(a \times b) + (c \times d)}$$

where a is the number of findspots of the focus having the trait (say 10), b is the number of occurrences of the trait in the focus findspots (10), c is the number of findspots of the focus lacking the trait (0), and d is the number of occurrences of the trait outside the focus (say 100,000). The computation would yield:

$$\frac{(10 \times 10) - (0 \times 100{,}000)}{(10 \times 10) + (0 \times 100{,}000)} = +1.00,$$

an index suggesting that there is a perfect association of the trait and focus although practically all of the specimens occurred in other foci. In Lehmer's index, a result of +1.00 always occurs when the trait in question occurs at all of the findspots of the focus, regardless of the proportions of the trait within and outside of the focus. It must be admitted that this is one way to look at association, but it does not seem to be the most informative way. Surely a better basis for judgment of archeological significance would result from computing two conventional indexes, one comparing proportions of specimens within and outside the findspots of the focus, the second comparing the proportions of findspots of various foci having and lacking the trait.

Two more matters remain for special comment. The first is Lehmer's use of the Southwestern term "branch" to indicate that the three foci at the Dodd site, the Phillips Ranch component, and other previously reported sites in the Middle Missouri region form a single and distinctive cultural tradition, the Ft. Pierre Branch. The new term serves a useful purpose because the categories of the Midwestern Taxonomic system are concerned only with formal relationships, not with combined formal and chronological relationships. However, I wish to enter an objection to the inclusion of the attribute "a limited geographic area" in the definition of the branch. This is a meaningless complication unless its inclusion can be justified on the ground that other attributes are associated with the geographical extent of sequent assemblages having the order of formal relationship implied by the branch concept. What would Lehmer do with another set of sequent components having exactly the same sort of formal relationship to each other as do the components of the Ft. Pierre Branch if they were scattered over an area three times as great? Would he deny that they formed a branch, thus obscuring the essential sameness of the formal and chronological relationship? I am told by those familiar with the Southwestern data that in that area the branch does in fact characteristically occupy a limited area, often the drainage of a single stream. But from the general point of view this is essentially accidental, and there is no reason to impose the concept on other areas. The core concept is that of a group of components sharing a common tradition and

occupying a span of time in sequence. The geographical extent of branches in the Plains is a problem to be investigated.

The final special comment deals with the problem of artifact, specifically pottery, typology. Lehmer's discussion is quite orthodox, that is to say (in the reviewer's opinion only) confused and operationally meaningless. This is not tantamount to asserting that Lehmer's types are meaningless, however, I would predict that any analysis would come out in pretty much the same way. The fault lies in the sphere of exposition, where Lehmer, like most archeologists, has a fairly definite idea of what the types are, but is unable to state clearly how he acquired the idea.

The summation of these comments on methodological aspects of the paper seems to indicate a chaotic pattern of criticism. In his statistical operations Lehmer is not orthodox enough, but in his typology and cultural classification he is too orthodox. This situation provides a clue to what I regard as the very real merit of his work, a willingness to search for better methods without falling into the booby trap of novelty for the sake of novelty. The special nature of this review obscures the fact that a very solid contribution to the Plains literature was made.

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